Society may well excite their sympathy. A moment's consideration will show that the progress of horticulture is largely based on the correct application of scientific principles. What is not so familar to most people is the extent of the obligation under which science generally lies to horticulture. Should any reader require an illustration of this, let him turn to the "Origin of Species," and specially to the "Variation of Animals and Plants under Domestication." There is scarcely a page in the chapters of those volumes relating to plants that does not abound in references to the practices and the discoveries of horticulturists. Fertilization, cross-breeding, hybridization, selection, grafting, the limits and nature of variation, are only a few of the subjects on which horticulture furnishes the largest and in many respects the most trustworthy body of evidence yet available. That these subjects are studied, and that the experiments are made, not so much from a scientific as from a utilitarian point of view, is surely no matter of reproach. On the contrary, it is the business of horticulturists to act as they do, but without the aid of a Society much of the experience gained would be lost to science. All this might be admitted as a general principle, but yet its concrete application to the Royal Horticultural Society might be from various causes inappropriate. The Society in question has, however, distinct and undoubted claims to recognition for the good work it has done in science for a long period of years. The services it has rendered to science by its collectors, and the still greater value of its work in all departments of practical horticulture, should have secured for it more sympathetic and respectful treatment from its landlords. Among Societies deserving of national support and encouragement on the ground of public utility, there are few, indeed, that have greater claims than this. For years it has unfortunately been hampered by the necessity of providing amusement for a body of Fellows and visitors who cared nothing whatever for horticulture in its higher aims. Now there is a chance of the Society bursting its bonds and confining itself to its proper work —the promotion of scientific and practical horticulture. The plan of catering for fashionable idlers has proved disastrous. While horticulture proper was starved, and thousands upon thousands of pounds were utterly wasted, the landlords retain the whole of the property on which their tenants expended so much, and the Society has to seek a new home. In spite of all this, however, a valiant attempt has been made throughout long years of depression to maintain the scientific traditions of the Society. There has always been a small body of Fellows who have been mindful of the obligation which Thomas Andrew Knight, so long the President of the Society, imposed upon his successors. Lindley for forty years maintained the scientific interests of horticulture in the Society, and he was assisted by Royle, by Bentham, and many others. Twenty years ago, or more, a Scientific Committee was appointed, and this body, recruited by new accessions each year, still continues its labours. Under its guidance experiments have been performed in the Society's Experimental Garden at Chiswick; under its sanction have been published numerous Reports of very great scientific interest and importance; and much more might and could have been done but for the lack of means, or rather their diversion for more questionable purposes. The Committee in question consists of some twenty or thirty naturalists of all denominations—botanists, chemists, geologists, entomologists—associated with amateur and professional gardeners interested in science. To this body are referred for discussion and investigation the most varied objects of natural history and vegetable pathology; before this body and its sister committees are brought all new introductions, whether of natural origin, or produced by the skill of the gardener, and which have any scientific interest. Sir Joseph Hooker is the Chairman of this Committee, the Rev. M. J. Berkeley was for many years its Secretary, and a large number of the most eminent biologists, chemists, and geologists have been or still are among its members, giving their services without fee or reward, simply in the interests of scientific horticulture. On these grounds, therefore, the sympathy and co-operation of those interested in science may be claimed on behalf of the Royal Horticultural Society. A new programme has been decided on in principle, a new home must be provided at once, for the purpose of the Society's meetings and for housing the Lindley Library. This library, it may be added, is held in trust for the benefit of the Society, and is from time to time enriched by donations and by purchase, so far as the very meagre income of the Trustees permits. The donations would be much more numerous were it generally known that the library, though primarily intended for the Fellows, yet is under certain restrictions available to outsiders, so that, though housed in the Society's rooms, it cannot be sold or made away with in any financial catastrophe which might overtake the Society. Such a fate, however, seems to be averted at present; the Society's debt is not large, and some members of the Council, or other friends, have made a good beginning by inaugurating a fund, to be used for the housing of the Society, so that ere long we may hope to see the old Society established on a more secure basis, and more potent than ever to advance those interests of science committed to its special keeping. MAXWELL T. MASTERS.

Classification of Clouds.

As one who has been engaged for nearly forty years in working up the materials for a monograph on clouds, I suppose I deserve the name of a "specialist in clouds" as much as any one. Yet I decline, for reasons which I will hereafter state in an appendix to my volume, to be altogether bound by the outlines of classification which my friends Prof. Hildebrandsson and the Hon. Ralph Abercromby appear to lay down (NATURE, December 8, p. 129 *et seq.*), although they adopt several of the names which come from my mint. I fully adopt the opinion implicitly held by Mr. Abercromby, and stated by my friend Captain Barker (*ibid.*)—from whose classification, however, I differ in one important point—that all ordinarily careful observers will readily comprehend the broad and simple distinctions expressed in any fairly good classification. Nevertheless, I believe that the apparently slow progress of this branch of research, and the tediousness of the work thrown upon the classifier, are matters on which we should congratulate ourselves, since every year adds something to our knowledge of those physical and structural processes which form the basis of all true classification; and I trust that some years may pass before an International Congress may attempt finally to set its seal upon any nomenclature or classification of clouds.

W. CLEMENT LEY.

Effect of Snow on the Polarization of the Sky.

THE polarization of the sky has been shown experimentally by Tyndall and theoretically by Lord Rayleigh to be due to fine particles suspended in the atmosphere. According to both, the sunlight scattered at right angles to its original direction by very small particles is completely polarized in a plane through the sun. In observation, however, we find the light from a region of the sky distant 90° from the sun is only partially polarized. This is due to that portion of the atmosphere being illuminated not merely by the sun, but also by the rest of the sky and the surface of the earth, and partly also no doubt to some of the particles not being sufficiently small compared with a wave-length. From these considerations we may expect that a fall of snow would cause a considerable diminution of the polarization. This expectation has been fulfilled in some recent observations of mine here at 6000 feet above sea-level. My polarimeter consists essentially of two piles of glass plates to depolarize the sky light; and a crystal and Nicol prism to test the depolarization. Owing to the strength of the polarization at this altitude, I find it necessary to use two piles of glass plates separated by two or three inches. This arrangement diminishes the number of double internal reflections, and so is a much more powerful polarizer or depolarizer than the same number of plates combined into one pile. As I have not seen this important practical consideration noticed before, I may point out that, in addition to the light refracted directly through the pile, there are a number of portions twice reflected. One of these for instance is reflected first at the second surface of the last plate, and secondly at the first surface of the last plate. The number of such twice-reflected portions for n plates is $n(2n-1)$. When, as in my instrument, the fixed pile is much inclined, no light can reach the edge after being reflected first by one pile and then by the other. If the two piles were combined into one, I should have 120 portions twice reflected; as it is, I have only 60. This increases the polarizing power of the instrument by at least one-third.

The crystal is a thick plate of Iceland spar cut so that the light passes along the optic axis. The fixed pile of three plates has its normal inclined at 47° to the axis of the crystal. The movable pile of five plates has an index attached, which gives the inclination of its normal to the axis of the crystal. This inclination is the reading of the polarimeter.

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St. Moritz lies on the northern slope of a valley running from south-west to north-east. At the beginning of the observations the opposite slope was buried in snow, but the northern slope both above and below the point of observation was almost free from snow. Thus the most brightly illuminated part of the ground surface was of a dull brown or gray colour. Under these circumstances, the reading was about 50° in the middle of the day, being a little higher earlier and later, viz. about 52° at 10 a.m. (date October 21 and 22). These readings, as well as those mentioned below, refer to the highest point of the sky, which is distant 90° from the sun, and were taken when the whole sky was free from cloud. On October 26, after a five-inch fall of snow, the reading was 41° at 10.15 a.m.

By October 29 most of the fresh snow had gone, and I found at 11.40 a.m. the reading as high as 48°. After this we had several feet of snow, and at 12.50 p.m. on November 13, the reading was again 41°. Each of these readings is the mean of four, and I find two readings of the same thing seldom differ more than 2°. Hitherto I have not been able properly to evaluate the readings of my instrument in absolute measure, though I hope to do so later. But to gain an approximate idea of their meaning, I have calculated the polarizing power of the two piles on the assumptions—first that Fresnel's laws of the reflection of polarized light are accurate, and secondly that the index of refraction of my plates is 1·52. We may consider the light from the sky as consisting of two parts completely polarized, one in the plane of the sun, and the other perpendicular thereto. The ratio of these parts is ·376 for the reading 40°, and ·271 for the reading 50°. Again we may divide the light into a part unpolarized and a part completely polarized in the plane of the sun. The ratio of these parts is ·546 for 40° and ·428 for 50°. So it seems fair to conclude that the light reflected from the fresh snow was sufficient to increase the unpolarized part of the sky light by more than a quarter.

JAMES C. MCCONNEL.

St. Moritz, Switzerland, December 10.

The Ffynnon Beuno and Cae Gwyn Caves.

I WILL answer Dr. Hicks's question in as few words as possible. Nothing is to be gained by terming me a "highly prejudiced" observer, or by saying my views are of "no consequence" and "not worth anything." Your readers can form their own conclusions on these points. I am not "highly prejudiced" against, neither have I any "bias against," the existence of pre-Glacial man or of his "migrations"; on the contrary, I favour these subjects.

I *did* see the section of drift exposed at the Cae Gwyn Cave, and I can hardly describe it (from my own point of view) without giving offence. My view is this: the section showed nothing but rain-wash derived from the closely-adjoining non-Glacial drift. The section showed a re-made deposit, horizontally stratified, and with stones resting on their flat sides. No doubt there were Glacial stones in the rain-wash, derived from the ever-shifting post-Glacial marine drift close by; the latter being merely a re-laid Glacial drift. Stones with Glacial scratches may be found in the lower gravels of the Thames.

To me, the caves and their surroundings are in the highest degree suspicious, and in size insignificant, and not comparable with large and typical caves. They are small and painfully narrow tortuous passages only, on a hill-side, and *close to the surface*. The lower cave is furnished with a very large hole, opening up to the surface just above; and the upper cave had at one time a similar opening. The post-Glacial drift above is always on the move, and every shower of rain brings it down with its derived stones.

Since writing to NATURE, in November 3, I have referred to some of the papers published on these caves. I turned first to the list of mammalian remains, only however to find that the animals (like the implements) are entirely *characteristic* of the most recent post-Glacial deposits. Even near London we get in gravels of no great comparative antiquity the bones of *Elephas antiquus*, but in the caves merely *E. primigenius* is found. As regards antiquity, the animals no doubt overlap at both ends of the scale, but their meaning, as found in these caves, points in one direction only, and that is to the most recent and not to the most remote of Palæolithic times. None of the cave mammals are *characteristic* of pre-Glacial deposits.

It would seem that Dr. Hicks does not realize the nature of Dr. John Evans's criticism. La Madelaine is the newest of caves, and represents the most recent of Palæolithic times: it is a kind of connecting link between Palæolithic and Neolithic times. Therefore, if Dr. Evans's criticism is taken with mine, the two *clearly prove* that there *is* a distinct chronological value in the classification, not that there is "no chronological value" as concluded by Dr. Hicks. Dr. Hicks also appears not to realize the fact that river-drift and cave implements do not only differ in roughness and abrasion but in style. The cave men used different implements from the river-drift men, they were changing from savagery to barbarism. If Dr. Hicks produces implements made by pre-Glacial men, he must show us something obviously older than the oldest river-drift tools, not fall back upon refined tools which are, to re-quote Dr. Evans, "precisely like many from the French caves of the reindeer period, such for instance as La Madelaine." If Dr. Hicks abandons his scraper, he is still in no better position, for his finely re-trimmed knife and the implement in the British Museum are identical in age and character with it. So are the flakes: the one with long narrow facets is characteristic of the latest, not of the earliest work. So is the pointed and drilled bone. No drilled bones have been found in moderately old river-gravels, and what is more, no instrument suitable for boring a small hole through bone has ever been found in such a gravel. Drilled bones and small flint drills belong to the very latest of Palæolithic times. In the remains of my own collection of Palæolithic implements I have here over a thousand examples of the major class, and an equal number of minor forms illustrative of the development of knife and scraper forms, but they give no support whatever to Dr. Hicks's conclusions; they all, in fact, point in a diametrically different direction. I am acquainted with Prof. Prestwich's views, and I believe I was the first person to find implements in the highest terraces of the Thames Valley; but I do not see that Prof. Prestwich's conclusions have any direct bearing on the Ffynnon Beuno and Cae Gwyn caves.

I do not suppose that any opinion of mine will influence Dr. Hicks, and I have no wish to influence him or any other observer. I merely wish to put on record the fact that, after many years' experience amongst drifts, and implements, and fossil bones, my conclusions are entirely opposed to Dr. Hicks's.

Dunstable. WORTHINGTON G. SMITH.

P.S.—Since the above has been in type, I have seen the report in last week's NATURE (p. 166), but I prefer to let my letter stand just as written before the report was seen by me. Prof. Hughes has cut away the geological and palæontological supports; I shall be content to resist the idea of the pre Glacial age of these caves on purely archæological grounds.—W. G. S.

The Planet Mercury.

THE planet observed on the mornings of December 7 and 9 by your correspondent "G. F. P." (NATURE, December 15, p. 151), was probably not Mercury but Jupiter, as these bodies were near together at the time, and the latter was by far the brightest and most conspicuous. The circumstances, described by "G. F. P.," under which the object was noticed render it certain that it could not have been Mercury, for the latter was decidedly small, and might have been easily overlooked on the several mornings I saw it early in the present month. Jupiter, on the other hand, was very bright and plain, and might easily attract attention in the way stated by your correspondent. On the 9th instant the two planets were about 3° apart, Jupiter being situated to the west of Mercury.

Had "G. F. P." really observed the latter planet, he would have instantly remarked its half-moon phase in his 3¾-inch telescope, and must have mentioned Jupiter, as well as Venus, as visible at the same time.

There is no difficulty in observing Mercury with the naked eye if the planet is carefully looked for in the proper spot, at the times of his eastern elongations in the first half of the year and at the western elongations in the last half. I have seen the planet on certainly more than fifty occasions. In May 1876 I noticed Mercury on thirteen different evenings. Sometimes the planet is quite conspicuous in the twilight as a naked-eye object.

W. F. DENNING.

Bristol, December 16.

Meteor of November 15.

IN NATURE of December 1 (p. 105) Mr. B. Truscott writes of a wonderfully fine meteor seen at Falmouth on the night of Tuesday, the 15th ult., and asks in effect if it was seen by other

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